

# *In Response*

## Ontological and Ideological Commitments in Behavior Analysis

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We concluded a previous article (Proctor & Weeks, 1988) by proposing three steps that behavior analysts should take if they wish their work to have further impact on the larger enterprise of scientific psychology. In a recent note, "Comments about the isolation of behavior analysis," Lee (1989) critically evaluated the recommended steps. The purpose of the present response is to provide clarifications of our intent in proposing these steps.

The first recommended step that Lee (1989) addressed was that behavior analysts should publish less in "in-house" journals and more in "mainstream" psychology journals. Her response was that publication in specialized journals is not uncommon and that the information is available to anyone who wishes to read it. Although it is true that many specialized psychology journals exist and that specialization does not necessarily result in insularity, the nature of these specializations is crucial. That is, the majority of specialized journals in psychology are organized according to content matter. Indeed, such content specialization prompted the division of the *Journal of Experimental Psychology* into four journals, according to content area. Even an interdisciplinary journal, like *Cognitive Science*, publishes articles of considerable theoretical and methodological heterogeneity. In contrast, the journals of behavior analysis are defined largely on

the basis of methodological mandate (see, e.g., Laties, 1987). This methodological specialization, coupled with a self-citation rate that is considerably higher than other archival journals (Bower & Hilgard, 1981), does promote the isolation of behavior analysis and is counterproductive, if the goal is to "become" psychology (Skinner, 1987).

We agree with Lee (1989) that behavior-analytic writings are available to anyone who wishes to read them and that researchers have an obligation to properly survey all relevant literature. However, authors have a similar responsibility to disseminate their work through outlets that will ensure that it receives exposure in the relevant literature and, thus, will be more readily available to interested parties. Therefore, although Lee is correct in noting that "we cannot blame . . . isolation entirely upon the contributors [to specialized journals]" (p. 85), neither can we blame investigators who are unaware of the contributions made by the members of a relatively small subdivision of psychology, when those members do not feel compelled to publish their work in mainstream outlets. In sum, it seems counterproductive for behavior analysts to be reluctant to publish in outlets that offer the potential for greater impact of their writings.

Lee's (1989) second comment concerned the "recommendation that behavior analysts should desist from rejecting cognitivism" (p. 85). Her reply was that if behavior analysts did not reject "cognitivism," then they would no longer be engaging in behavior analysis. However, in paraphrasing our recommendation, Lee has missed its essence. The original passage read: "behavior an-

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alysts should *join* [italics added] the rest of the psychological community in the ongoing *pursuit of scientific knowledge* [italics added], without having the predetermined agenda of '*supplanting* [italics added] cognitive explanations' (Johnston & Shook, 1987, pp. 231-232)" (Proctor & Weeks, 1988, p. 139). Our point is that scientific explanations of any type, cognitive, behavior-analytic, or otherwise, should not be supplanted on ideological grounds, particularly given that "supplant" can mean "to supersede (another) esp. by force or treachery" (*Webster's Ninth New Collegiate Dictionary*, 1987). Also, by "joining in the pursuit of scientific knowledge," we did not mean to imply that behavior analysts should abandon their ontological commitment, but that they should avoid an ideology that denies the possible validity and utility of other approaches to the study of human behavior. To the extent that the ontological commitment of behavior analysts precludes any provision for considering the possible validity of alternative approaches, the scientific legitimacy of behavior analysis is weakened.

Next, Lee (1989) evaluated the recommendation "that behavior analysts should acquaint themselves with cognitive psychology" (p. 85). Her reply was that "it is difficult to see what we would gain from acquainting ourselves with cognitivism" (p. 86). Once again, Lee's description of our recommendation loses the spirit in which it was offered. The original passage read: "behavior analysts should *cease denying* the *reality* [italics added] of the cognitive revolution and familiarize themselves with the major advances that have occurred in psychological research" (Proctor & Weeks, 1988, p. 138). The first point to note is that we suggested only that behavior analysts become *familiar* with psychological research, broadly defined. Moreover, the suggestion to cease denying the reality of the cognitive revolution was embedded in a more detailed discussion of current trends in scientific psychology. That is, our article was a response to Harzem's (1987) directive that behavior-analytic

research should address the topics of (a) effective training techniques and (b) decision-making phenomena, both of which account for substantial portions of the literature in contemporary cognitive psychology. In light of Harzem's assertion that psychology has been remiss in studying these issues, and the inaccuracies in his portrayal of scientific psychology, we recommended that behavior analysts familiarize themselves with the pertinent literature first-hand. If Harzem's (1987) article is representative of behavior analysts' knowledge regarding contemporary psychology, then there is much to be gained by becoming acquainted with the advances made by other approaches.

Apparently, Lee (1989) interpreted our recommendations as endorsing a particular approach. But that was not our intent. Rather, we acknowledge the existence and possible utility of a variety of approaches within psychology, including but not limited to cognitive psychology and behavior analysis. Moreover, we view any of these efforts that take a scientific approach to the study of human behavior as falling under the umbrella of behaviorism. Thus, we regard ourselves as scientists first, experimental psychologists second, and behaviorists third. As behaviorists, we adhere to the tradition of empiricism and the scientific goals of understanding, prediction, and control of behavior (e.g., Kerlinger, 1986). Many of our inquiries into human behavior are organized around the notion of humans as processors of information. However, other approaches provide different organizational tools, each with advantages and disadvantages.

Lee (1989) is prepared to acknowledge that individuals other than behavior analysts have observed many interesting behavioral phenomena. However, she charges that behavior analysts do not need to explain the data that have been collected in the examination of these phenomena, because the data "were collected for nonbehavioral purposes and with nonbehavioral methods" (p. 86). But, as stated by Sidman (1960):

Problems arise, however, when scientists make value judgments about the reasons for performing ex-

periments and then use such judgments as a basis for accepting or rejecting the data. Good data are always separable with respect to their scientific importance, from the purposes for which they were obtained. (p. 3)

Yet, in Lee's (1989) judgment, behavior analysts should "spend [their] energies contributing to a science of behavior rather than arguing about the interpretation of cognitive *data*, which are often of interest only in relation to cognitive theory if only because they are laden with cognitive theory" (p. 86). The simple characterization of data as "cognitive" cannot serve as a basis for disregarding the massive data base that exists within psychology. This data base is not "cognitive" but behavioral. Moreover, the data collected from any field of inquiry are not theory laden, in and of themselves. Rather, it is the explanations of the data that are ingrained with theory. If behavior analysts wish to claim that they possess an all-encompassing account of human behavior (e.g., Harzem & Williams, 1983), then scientific criteria require that their task include accounting for all the relevant behavioral data, even if the data are accompanied by theoretical explanations that are not preferred by behavior analysts. Thus, a behavior-analytic explanation that both encompasses existing behavioral data and is grounded in new empirical evidence would be welcome. We see this as one way to promote a meaningful convergence of interests in the long run.

Lee's comments regarding the roles of data and explanations in psychological inquiry seem to reflect a more general confusion among behavior analysts. Behavior analysts seem to assume that, in contemporary psychology, cognitive constructs are necessary rather than only sufficient as explanatory devices. But contemporary psychology allows only for the *inclusion* of such constructs when explaining behavioral data. Moreover, cognitive constructs are used judiciously and only when their inclusion enables appreciably more accurate prediction of future behaviors. Realizing that only the explanations given to data, and not the data themselves, can be "cognitive" enables

progress to be made through useful and critical evaluations of alternative explanations, cognitive or otherwise. Moreover, we regard any systematic challenges by behavior analysts to the theoretical explanations generated from other approaches to be invaluable. Thus, by working in concert to understand behavioral phenomena, the most accurate and parsimonious accounts will be selected.

In closing, we would like to comment on Lee's (1989) use of the term "cognitivism," because it is a term that we did not use. This term is used infrequently within experimental psychology (see, e.g., Hilgard, 1987), because cognitive psychology is aligned closely with behaviorism in its empirical approach to the study of human behavior. "Cognitivism" is used more frequently when discussing philosophical issues within the field of cognitive science (e.g., Pylyshyn, 1984), in which the central focus is the interdisciplinary study of cognition in its own right. Whereas, issues regarding the epistemology of cognitivism are of central concern to cognitive scientists, these issues are only ancillary to the activities of cognitive psychologists in their explanations of behavior.

Our position is that the acquisition of knowledge within a scientific discipline can proceed relatively independently from overriding philosophical debates. That is, progress is made by allowing the empirical evidence, rather than philosophical predispositions, to guide research efforts. This point is captured succinctly by Donald Broadbent (1961):

Nobody can grasp the nature of things from an armchair, and until fresh experiments have been performed we do not know what their results will be. The confident dogmatisms about human nature which fall so readily from pulpits, newspaper editorials, and school prize-givings are not for us. Rather, we must be prepared to live with an incomplete knowledge of behavior but with confidence in the power of objective methods to give us that knowledge some day. (pp. 200-201)

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